general rigidity. Another, however, has the following syndrome, constantly repeated. The ocular spasm, or posture, directs the eyes downwards and slightly to one side. At the same moment speech is lost ("the words come to my mouth, but I can't get them out"), the neck flexes slightly, with a slightly increased rigidity of the neck muscles, and if one arm be moved back and forth passively before and during the attack, there can be no doubt about an increase of rigidity with the onset of the attack. In this case, too, there is usually an active spasm of the eyelids during the crisis.

In interpreting spasmodic muscular contractions and postures as "release phenomena" it is difficult to know where to stop; but in view of the type of case illustrated in this patient it is difficult to abandon a conception of the ocular attack as being linked with the other inhibitions of motor function which, together with itself, are frequently linked, by coincidence in time at least, with variations of rigidity. It is also difficult to feel quite convinced that all these events can find their explanation along a line of association, or of dissociation, of the function of sleep.—I am, etc.,

London, W.1, Nov. 24th. RANYARD WEST.

DEATHS ASSOCIATED WITH ANAESTHESIA

SIR,—Dr. Frederick L. Hoffman's letter in your issue of November 28th is interesting, but, I am afraid, not illuminating. Anaesthetists would wish to know before taking his figures too seriously to heart: (1) What is the death rate (under or associated with anaesthesia) per 100,000 *anaesthetics given* in the United States of America and England and Wales? (2) What are the laws governing those whose duty it is to report such cases in the two countries? (3) How far are those laws conscientiously obeyed? Without answers to these questions no proper conclusions can be arrived at from Dr. Hoffman's figures.—I am, etc.,

London, N.W., Dec. 1st. RAYMOND E. APPERLY.

ALLERGY AND THE "PROTEOSE"

SIR,—In reply to Dr. John Freeman's letter, published in your issue of November 28th, I should like to draw attention to the following points.

1. In the isolation of a new substance it is an invariable experience that the original technique used for isolation has to be modified in the light of subsequent experience. The criterion of potency of "proteose" does not depend on its quantity, but on its power of reaction with antibodies. Whereas a normal person excretes a certain quantity of "proteose," he never, so far as I am aware, gives a positive skin test to this substance, neither does its injection cause symptoms.

2. Dr. Barber will, I believe, explain to Dr. Freeman the technique which should be employed to elicit a specific reaction in the eczema-asthma syndrome.

3. The case Dr. Freeman quotes as giving a negative reaction to "proteose" by his technique was used for the experiments in urinary antigens reported to the Royal Society of Medicine (*Proc. Roy. Soc. Med.*, Section of Dermatology, July, 1931, xxiv, 55). In Fig. 1 the experiment with "proteose" labelled "Proteose, Dr. F.'s case," is the identical case which Dr. Freeman quotes as giving a negative skin reaction. Guinea-pig experiments were used in this connexion, as they are recognized to be more specific than skin tests.

4. In some work in process of publication, Dr. Conybeare, working at the Asthma Clinic, Guy's Hospital, undertook an independent investigation, using a "proteose" isolated from a patient suffering from hay-fever-asthma, and tried the effect of intradermal tests with this patient's "proteose" on (*a*) pollen-sensitive subjects, (*b*) asthmatic subjects who were not pollen-sensitive, (*c*) normal controls. In this series of approximately 100 cases it was found that 80 per cent. of the pollen-sensitive patients gave positive skin tests to the hay-fever "proteose," whereas none of the normal controls did so. Some 20 per cent. of the asthmatic patients who were not pollen-sensitive gave positive tests. It is interesting that some of these patients were clinically hay-fever subjects, although they gave negative skin tests to pollen. Tracings of all these skin tests were kept, and will be available for inspection by anyone interested as soon as Dr. Conybeare, who is at present in America, is able to return them.

It seems to me that the question of the specificity of the "proteose" is of sufficient importance to justify further investigation, and I should be pleased to arrange a demonstration of the skin tests in which Dr. Freeman finds difficulty, if he will be good enough to indicate the time and place that will be suitable for him.—I am, etc.,

London, W.1, Dec. 2nd. G. H. ORIEL, M.D.

ASTHMA STATISTICS

SIR,—Your editorial review of Gram's "Danish asthma statistics" is very interesting, but the age incidence varies so greatly from that found to exist in my own series of cases that one may doubt whether the figures taken from a "sickness insurance organization" give us much idea of the age incidence in Denmark as a whole.

Reduced to percentages, Gram finds that in his series of cases asthma started in the first five ten-yearly periods of life, in the proportions of 12, 13, 24, 23, and 16 per cent. of his patients respectively. The age incidence in my own series of 1,000 cases (already published) was totally different—namely, 40 started asthma in the first ten years of life, 12, 15, 13, and 9 in the subsequent decades. In a second series of 1,000 cases my figures are: 39, 13, 14, 14, and 11. Thus I differ from Gram. My finding is that asthma most commonly commences in the first ten years of life.

On the question of sensitization his figures agree almost exactly with mine. Applying my percentages to the numbers of patients he details to each ten-yearly period, I should expect ninety-six of them to give reactions to the dermal tests. As a matter of fact he found ninety-one to be sensitive to this method. Thus the skin reactions, which were at one time so much belittled, again receive confirmation.

Probably a series taken from our own national insurance service would be more correctly parallel with Gram's cases, but in neither case would they give the asthma figures for the whole population. In short, I see no reason to suspect any difference in the age incidence of asthma in two places so similarly situated as Denmark and England.—I am, etc.,

London, W.1, Dec. 7th. FRANK COKE, F.R.C.S.

"REAL PAIN"

SIR,—In your report of the Royal Society of Medicine's discussion on nervous dyspepsia (*Journal*, December 5th, p. 1035), Dr. Robert Hutchison is represented as having said that "the presence of real as distinct from imagined pain might almost be said to negative a diagnosis of nervous dyspepsia." Possibly unavoidable condensation of his remarks has eliminated whatever he may have said to define and explain the distinction to which he refers. This is unfortunate, for it would be interesting to know by what criterion he estimates the "reality" of pain, dyspeptic or otherwise. A patient may be imaginative or mistaken as to the reasons he gives for his pain or as to the organ which he states to be involved: on such matters he is not competent to judge. But surely what makes pain "real" is simply painfulness, and on that point the patient is the only possible judge. Failure